

intended to describe the real phenomena, may I ask you to allow me a few lines of space for the following:—

The real sun was surrounded at a short distance by a halo or rainbow circle of great beauty, with a mock sun of the same apparent magnitude and brightness on the right and left; and *partially* formed suns above and below the ring: all of them being slightly opalescent. From the two perfected suns proceeded cones of intense light, about 3 diameters of the sun in length, and with their apices pointing east and west. These were rather more opalescent than the mock suns from which they seemed to originate. A second rainbow ring at a considerable distance outside of these extended to the zenith. The period of greatest beauty and brightness, when they were as rich in colouring as a real rainbow, lasted about 5 minutes. I was able to watch the whole of the phenomena from a little after 4 to nearly 6 o'clock.

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# PHYSIOLOGICAL SELECTION: AN ADDITIONAL SUGGESTION ON THE ORIGIN OF SPECIES<sup>1</sup>

## III.

### ARGUMENT from the Prevention of Intercrossing.—

This argument is the same from whatever cause the prevention of intercrossing may arise. Where intercrossing is prevented by geographical barriers or by migration, it is more easy to prove the evolution of new species as a consequence than it is when intercrossing is prevented by physiological barriers; for in the latter case the older and the newer forms will probably continue to occupy the same area, and then there will be no independent evidence to show that the severance between them was due to the prevention of intercrossing. Nevertheless, all the evidence I have of the large part that geographical barriers have played in the evolution of species by preventing intercrossing with parent forms goes to show the probable importance of physiological barriers when acting in the same way. Hence it will be better to postpone this line of argument in favour of physiological selection until the appearance of my next paper, where I shall hope to show, from evidence furnished by the geographical distribution of species, how predominant a part the prevention of intercrossing has played in the evolution of species. Here, therefore, I will merely remark that wherever intercrossing with parent forms is prevented, in the proportion that it is prevented a better opportunity is given to natural selection for seizing upon any beneficial variations that may happen to arise. On this account physiological selection probably lends important aid to natural selection, thus becoming indirectly instrumental in the evolution of useful as well as of useless structures.

There is also another respect in which these two kinds of selection probably co-operate. For Mr. Darwin shows that "it would be clearly advantageous to two varieties, or incipient species, if they could be kept from blending, on the same principle that, when man is selecting at the same time two varieties, it is necessary that he should keep them separate." But he proceeds to show that this advantage cannot be conferred by natural selection, and hence that the sterility which is so generally characteristic of species cannot be attributed to this agency. We have, however, just seen that this sterility is in all likelihood due to physiological selection; and therefore, if it be true, as Mr. Darwin thought, that "it would profit an incipient species if it were rendered in some slight degree sterile with its parent form," physiological selection and natural selection may mutually assist one another. For, although the benefit of this sterility could not have been initially conferred by natural selection, yet when it once arises from an independent variation in the reproductive system, there is no reason why it should not forthwith be favoured by natural selection, just as is the case with advantageous variations in general.

<sup>1</sup> Abstract of a Paper read before the Linnean Society on May 6, by George J. Romanes, M.A., LL.D., F.R.S. &c. Continued from p. 340.

Feeling how grave a difficulty was presented to his theory of the origin of species by the general sterility of species, Mr. Darwin was extremely anxious to find some way in which natural selection might be seen to have brought about this result. Had it occurred to him that this result was probably nothing more than the necessary expression of a particular kind of variation on the part of the reproductive system, I cannot doubt that he would have felt the theory of natural selection to have been relieved of one of its greatest disabilities.

### Argument from the Inutility of Specific Differences.—

After what has already been said on this subject, I will here only deal with one question, namely, Why is it that apparently useless structures occur in such profusion among species, in much less profusion among genera, and scarcely at all among families, orders, and classes? It may be answered that the points wherein species differ from species are usually points of smaller detail than those which distinguish genera, families, &c., and thus may well actually be as a rule less useful, although still not absolutely useless: natural selection, it may be urged, is better able than is the naturalist to diagnose utility. But here again we have a most unwarranted appeal to the argument from ignorance; whereas, according to my view, it is quite intelligible that when a varietal form is differentiated from its parent form by the bar of sterility, isolation, or migration, any little meaningless peculiarities of structure (or of instinct<sup>1</sup>) should at first be allowed to arise, but should eventually be eliminated as so much surplusage in the struggle for existence, by economy of growth, or even by independent variation when undirected by natural selection. A greater or less time would in different cases be required to effect this reduction, and thus we can understand why they are sometimes allowed to persist into genera, but rarely into families.

Again, if apparently useless specific characters (whether these be new structures or modifications of old ones, slight changes in form, colour, and so forth) are thus regarded as really useless, we should expect that they ought to be of a kind which do not impose much physiological tax upon the organism, since otherwise natural selection would not have allowed them to become so much as specific characters. Well, I have applied this test, and find it is a most general rule that specific characters the utility of which cannot be perceived are such as do not impose any considerable demand for nourishment: either on account of their small size or of their organically inexpensive material, they do not impose much tax upon the organism. Now it is obvious that there can be no connection between utility as disguised and smallness of size or inexpensiveness of material; while it is no less obvious that there is a close connection between these things and a real inutility.

Lastly, our domesticated varieties occasionally exhibit well-marked and more or less constant characters of a useless kind. Here there can scarcely be any question about the genuineness of the inutility, seeing that the characters have arisen only under domestication, or in the absence of any struggle for existence. Yet these structures are sometimes of the most curious and complex morphology—even more so than innumerable apparently useless structures in the case of natural species.<sup>2</sup>

*Argument from Divergence of Character.*—Any theory of the origin of species in the way of descent must be prepared with an answer to the question, Why have species multiplied? Why have they not simply become transmuted in linear series instead of ramifying into branches? This question Mr. Darwin seeks to answer

<sup>1</sup> For instances of useless instincts see Mr. Darwin's posthumous essay published in my "Mental Evolution in Animals." It is suggestive in the present connection that, just like useless structures, useless instincts, so far as I can find, only occur in species and genera: never in families, orders, or classes.

<sup>2</sup> For a good instance of this see "Variation of Plants and Animals under Domestication," vol. i. pp. 78-79.

"from the simple circumstance that the more diversified the descendants from any one species become in structure, constitution, and habits, by so much will they be better enabled to seize on many and widely diversified places in the economy of nature, and so be enabled to increase in numbers."<sup>1</sup> And he proceeds to illustrate this principle by means of a diagram, showing the hypothetical divergence of character undergone by the descendants of seven species. Thus, he attributes divergence of character exclusively to the influence of natural selection.

Now, this argument appears to me unassailable in all save one particular; but this is a most important particular: the argument wholly ignores the effect of intercrossing with parent forms. Granting to the argument that intercrossing with parent forms is prohibited, and nothing can be more satisfactory. The argument, however, sets out with showing that it is in limited areas, or in areas already overstocked with the specific forms in question, that the advantages to be derived from diversification will be most pronounced. Or, in Mr. Darwin's words, it is where they "jostle each other most closely" that natural selection will set a premium upon any members of the species which may depart from the common type. Now, inasmuch as this jostling or overcrowding of individuals is a needful condition to the agency of natural selection in the way of diversifying character, must we not feel that the general difficulty from intercrossing previously considered is here presented in a special and aggravated form? At all events, I know that, after having duly and impartially considered the matter, to me it does appear that, unless the swamping effects of intercrossing with the parent form on an overcrowded area is in some way prevented to begin with, natural selection could never have any material supplied by which to go on with. Let it be observed that I regard Mr. Darwin's argument as perfectly sound where it treats of the divergence of *species* from one another—i.e. of the rise of genera, families, &c.; for then physiological barriers are present to prevent intercrossing. But in applying the argument to explain the divergence of *individuals* into *varieties* it seems to me that here, more than anywhere else, he has lost sight of the formidable difficulty in question. For in this particular case so formidable does the difficulty seem to me, that I cannot believe natural selection alone could produce any divergence of character so long as all the individuals on an overcrowded area occupy that area together. Yet if any of them quit that area, and so escape from the unifying influence of free intercrossing, these individuals also escape from the conditions which Mr. Darwin names as those that are needed by natural selection in order to produce divergence. Therefore it appears to me that, under the circumstances supposed, natural selection alone could not produce divergence; the most it could do would be to change the whole specific type in some one direction (the needful variations in that one direction being caused by some general change of food, climate, habit, &c., affecting a number of individuals simultaneously), and thus induce transmutation of species in a linear series—each succeeding member of which might supplant its parent form. But, in order to secure diversity, multiplication, or ramification of species, it appears to me obvious that the primary condition required is that of preventing intercrossing with parent forms at the origin of each branch—whether the prevention be from the first absolute, or only partial. And, after all that has been previously said, it is needless again to show that the principles of physiological selection are at once the only principles which are here likely to be efficient, and the principles which are fully capable of doing all that is required. For species, as they now stand, unquestionably prove the fact of ramification; and it appears to me no less unquestionable that ramifi-

cation, as often as it has occurred, can only have been permitted to occur by the absence of intercrossing with parent forms. But apart from geographical barriers (which, according to Mr. Darwin's argument, would be inimical to the divergence of character by natural selection), the ramification can only take place as a consequence of physiological selection, or as a consequence of some change in the reproductive system which prevents intercrossing with unchanged (or differently changed) compatriots. But when once this condition is supplied by physiological selection, I have no doubt that divergence of character may then be promoted by natural selection, in the way that is explained by Mr. Darwin.

From which it will be seen that the theory of physiological selection has this advantage over the theory of natural selection in the way of explaining what Mr. Darwin calls diversification of character, or what I have called the ramification of species. This diversification or ramification has reference chiefly to the secondary specific distinctions, which, as we have seen, the theory of natural selection supposes to be the first changes that occur, and, by their occurrence, to induce the primary distinction of sterility. My theory, on the other hand, inverts this order, and supposes the primary distinction to be likewise (in most cases) the primordial distinction. Now, the advantages thus gained are twofold. In the first place, as just shown, we are able to release the principle of natural selection from what appears to me the otherwise hopeless difficulty of effecting diversification of character on an overcrowded area with nothing to prevent free intercrossing. And, in the next place, as we can now see, we are able to find an additional reason for the diversification of character, over and above the one that is relied upon by Mr. Darwin. For, by regarding the primary distinction of sterility as likewise the primordial distinction, we are able to apply to an incipient variety, inhabiting even an overcrowded area, the same principles which are known to lead to diversification by geographical barriers or by migration, as previously explained. In other words, if once we regard the primary distinction of sterility as also the initial distinction, instead of the incidental result of secondary distinctions, Mr. Darwin's argument touching the causes of diversification is not merely saved: it is notably extended by the addition of an independent principle, which, as we know from other evidence, is a principle of high importance in this respect.

*Argument from Geographical Distribution.*—The body of evidence under this head is too large to be given in an abstract; but the following are some of the chief points.

Mr. Darwin took a great deal of trouble to collect evidence on the two following facts, namely, (1) that "species of the larger genera in each country vary more frequently than the species of smaller genera"; and (2) that "many of the species included within the larger genera resemble varieties in being very closely, but unequally, related to each other." By larger genera he means genera containing many species, and he accounts for these general facts by the principle "that where many species of a genus have been formed, on an average many are still forming." But how forming? If we say by natural selection alone, we should expect to find the multitudinous species differing from one another in respect of features presenting utilitarian significance; yet this is precisely what we do not find. For Mr. Darwin's argument here consists in showing that "in large genera the amount of difference between the species is often exceedingly small, so that in this respect the species of the larger genera resemble varieties more than do the species of the smaller genera." Therefore the argument, while undoubtedly a very forcible one in favour of the fact of *evolution*, appears to me scarcely consistent with the theory of *natural selection*. On the other hand, the argument tells strongly (though unconsciously) in favour

<sup>1</sup> "Origin of Species," p. 27.

of physiological selection. For, the larger a genus, or the greater number of species it contains, the greater must be the opportunity afforded for the occurrence of that particular kind of variation on which the principle of physiological selection depends. All the species of a genus may be regarded as so many varieties which have already been separated from one another physiologically: therefore each of them may now constitute a new starting-point for a further and similar separation—particularly as, in virtue of their previous segregation, many of them are now exposed to different conditions of life. Thus, it seems to me, we can well understand why it is that genera already rich in species tend to grow still richer; while such is not the case in so great a degree with genera that are poor in species. Moreover, we can well understand that, multiplication of species being in the first instance determined by changes in the reproductive system alone, wherever a large number of new species are being turned out, the secondary differences between them should be “often exceedingly small”—a general correlation which, so far as I can see, we are not able to understand on the theory of natural selection.

Another general fact mentioned by Darwin, and now well recognised by all naturalists, is that closely allied species, or species differing from one another in trivial details, usually occupy contiguous areas; or, conversely stated, that contiguity of geographical position is favourable to the appearance of species closely allied to one another. Of course this fact speaks in favour of evolution; but where the question is as to method, I confess that the theory of natural selection appears to me wholly irrelevant. For, in most of the numberless cases to which I allude, the points of minute detail wherein the allied species differ in respect of secondary distinctions, are points which present no utilitarian significance. And, as previously argued, it is impossible to believe that there can be any general or constant correlation between disguised utility and insignificance of secondary distinction.

Now the large body of facts to which I here allude, but which I have not space to detail, appears to me to constitute perhaps the strongest of all my arguments in favour of physiological selection. Take, for instance, a large continental area, and follow across it a chain of species, each link of which differs from those on either side of it by the most minute and trivial distinctions of a secondary kind; but all the links of which differ from one another in respect of their reproductive systems, so that no one member of the series is perfectly fertile with any other member. Can it be supposed that in every case this constant primary distinction has been superinduced by the trivial secondary distinctions, distributed as they are over different parts of all these kindred organisms, and yet nowhere presenting any but the most trifling amount of morphological change? Or, even if we were to suppose this, we have still to meet the question, How were all these trifling changes produced in the face of free intercrossing on the continental area? Certainly not by natural selection, seeing that they are all useless to the species presenting them. Let it then be by changes in the conditions of life, whether of food, of climate, or of anything else. I can conceive of no other alternative. Yet, if we accept this alternative, we are but espousing—in a disguised and roundabout way, to be sure—the theory of physiological selection. For we are thus but hypothetically assigning the causes which have induced the primary distinction in each case, or the causes which have led to the mutual sterility. For my own part, I believe that the assignation would be, in the great majority of such cases, incorrect. That is to say, I do not believe that in the great majority of such cases the trivial secondary distinctions—however these were caused—can have had anything to do with the great primary distinction. What I believe is that all the closely-allied species inhabiting our

supposed continent, and differing from one another in so many points of minute detail, are but so many records of one particular kind of variation having taken place in the reproductive systems of their ancestors, and which, so often as it did take place, necessarily gave birth to a new species. The primary distinction thus became the constant distinction, simply because it was in virtue of this distinction—or in virtue of the variation which first originated this distinction—that the species became species; and the secondary distinctions thus became multitudinous, minute, and unmeaning, simply because they were of later origin, the result of spontaneous variability, unchecked by intercrossing with the parent forms, and, on account of their trivial (*i.e.* physiologically harmless) nature, unchecked also by natural selection, economy of growth, or any other principle which might have prevented spontaneous variability of any other kind.

There are many other general facts relating to geographical distribution which lend the strongest countenance to the theory of physiological selection—in particular I may mention the difficulty which Mr. Darwin experiences in accounting for the absence or rarity of transitional varieties between species inhabiting contiguous areas (*loc. cit.*, p. 134), which is just what might have been expected on my theory—but it is time that this abstract should draw to a close.

*Relations between the Theories of Natural Selection and Physiological Selection.*—The two theories resemble one another in the kind of evidence by which they are each supported. For in neither case is this evidence that of direct observation of the transmutation of species under the influence of the agency supposed: the evidence in each case consists in first proving the facts on which the principle depends, and then showing that the phenomena of organic nature are such as they ought to be if the principle in question has had any large share in their production. But the two theories differ in that while natural selection is a theory of the origin of genera, families, orders, and classes even more than it is of the origin of species; the theory of physiological selection is almost exclusively a theory of the origin of species. Again, the latter theory differs from the former in that the variations on the occurrence of which it depends are variations of a comparatively useless, or non-adaptive, kind. Nevertheless, physiological selection must be quite as vigilant as natural selection, and it seizes upon the comparatively useless variation of sterility with even more certainty than natural selection can seize upon any useful variation. Lastly, as will have been gathered from the foregoing abstract, the two theories are in no way opposed to one another: they are, in fact, complementary, and the principles with which they have to deal co-operative. For, on the one hand, without the assistance of physiological selection, natural selection would, I believe, be all but overcome by the adverse influences of free intercrossing—influences all the more potent under the very conditions which are required for the multiplication of species by divergence of character. On the other hand, without natural selection, physiological selection would be powerless to create any differences of specific type other than those of mutual sterility and trivial details of structure, form, or colour—differences wholly without meaning from a utilitarian point of view. But in their combination these two principles appear to me able to accomplish what neither can accomplish alone—namely, a full and satisfactory explanation of the origin of species.

*Conclusion.*—It has not been possible to do justice to the theory of physiological selection within the limits of this abstract. But perhaps enough has been said to show that there is a great deal of evidence in its support; that by regarding mutually sterile species as records of variation in reproductive systems, we are at work, so to speak, on the foundation of the matter; and that we are thus able to explain a number of general facts which do not

admit of being explained by any previous theory. It only remains to add that, if true, the present theory ought to admit of experimental verification. Let well-marked natural varieties of plants growing on the same area be systematically tested with regard to their relative degrees of fertility, first within themselves, and next towards one another: let these experiments be made in successive years over a number of natural varieties, by carefully-conducted artificial fertilisation, and by counting the seeds and tabulating the results. In this way experimental evidence would probably be obtained of degrees of sterility between even slight though constant varieties growing on the same areas; and, if so, such evidence would serve as further proof of the present theory. But experiments of this kind, in order to be satisfactory, ought to be conducted by a number of observers in different geographical areas; and my object in publishing so lengthy an abstract of my views in this periodical is that of inducing naturalists in other parts of the world to co-operate with me in carrying out this research. The paper itself, which furnishes fuller particulars as to the way in which such experiments should be carried out, is published in a separate form by the Linnean Society.

### THE WOODEND COLLIERY EXPLOSION

*QUI s'excuse s'accuse* will occur to the minds of many who have followed the details of the disastrous explosion which took place at Woodend or Bedford Colliery on Friday last. We read in the *Times* of the 16th inst.:—"The Four-foot or Crombonke Mine is a very dusty one, and it is considered that at the Woodend pit the dust has increased the extent of the damage." "But to water the mine, as suggested by the Commission, would here be a very difficult operation, because the floor of the mine consists of a species of fire-clay which, as it absorbs the water, causes a lifting of the ground, and so prevents mining operations being conducted." Inasmuch, however, as the floor of perhaps ninety-nine out of every hundred mines consists of the same kind of material, the same argument against watering would hold equally good in most cases, and, if it is allowed to pass, this recommendation of the Commissioners is likely to come to nothing. It has been pointed out more than once in *NATURE* that the amount of water required to lay the dust is very small—far less than would be necessary to materially affect the floor of a mine in the manner suggested, and it would perhaps be wiser to try the effect in the first place and judge by results rather than to meet the proposition with a simple *non possumus*. We speak thus plainly here, because many of the witnesses who gave evidence before the Commissioners brought forward the very same argument with the same degree of plausibility, and we have reason to believe without having put the matter to a practical test. Many of those who now water regularly, for the express purpose of laying the dust on floors consisting of fire-clay, admit that the water produces no appreciable difference when properly and carefully distributed.

The bursting of the gauze of a safety-lamp, described by one of the survivors, is so contrary to all reason and experience that it cannot be accepted as an explanation of the origin of the explosion. Hundreds, if not thousands, of safety lamps are placed in explosive gas every day when the mines are being tested for the presence of fire-damp, and yet no parallel case has ever been recorded. Under these circumstances we prefer to attribute it to some other still unknown cause. We have yet to learn whether shots were fired in the mine, and if so we have probably not far to look for the explanation.

Up to the present all we know with certainty is that the mine produced very little gas, that it was dry and dusty, and that the explosion was violent but not universal. It would be most interesting, as well as instructive,

to ascertain whether any natural local dampness curtailed its extent; but as this is a feature that has not hitherto attracted or received much attention, we are not sanguine that it will be carefully inquired into in the present case. We shall, however, watch the future course of the inquiry, and perhaps again comment upon it for the benefit of our readers.

W. G.

### ON THE DIFFERENTIAL EQUATION TO A CURVE OF ANY ORDER

TO Mr. Samuel Roberts (see Reprint of *Educational Times*, vol. x. p. 47) is due the credit of having been the first to show that a direct method of elimination properly conducted leads to the differential equation for a cubic curve: but he has not attempted to obtain the general formula for a curve of any order. By aid of a very simple idea explained in a paper intended to appear in the *Comptes rendus* of the Institute, I find without calculation the general form of this equation. The left-hand member of it may be conveniently termed the differential *criterion* to the curve. One single matrix will then serve to express the criteria for all curves whose order does not exceed any prescribed number. For instance, suppose we wish to have the criteria for the orders 1, 2, 3, 4:—

Let  $m\mu$  be used in general to denote the coefficient of

$$h^m \text{ in } \left( \frac{1}{1.2} y'' h^2 + \frac{1}{1.2.3} y''' h^3 + \frac{1}{1.2.3.4} y^{(4)} h^4 + \dots \right)^m.$$

Write down the matrix—

|      |      |      |      |      |      |      |      |      |      |
|------|------|------|------|------|------|------|------|------|------|
| 2'1  | 3'1  | 3'2  | 4'1  | 4'2  | 4'3  | 5'1  | 5'2  | 5'3  | 5'4  |
| 3'1  | 4'1  | 4'2  | 5'1  | 5'2  | 5'3  | 6'1  | 6'2  | 6'3  | 6'4  |
| 4'1  | 5'1  | 5'2  | 6'1  | 6'2  | 6'3  | 7'1  | 7'2  | 7'3  | 7'4  |
| 5'1  | 6'1  | 6'2  | 7'1  | 7'2  | 7'3  | 8'1  | 8'2  | 8'3  | 8'4  |
| 6'1  | 7'1  | 7'2  | 8'1  | 8'2  | 8'3  | 9'1  | 9'2  | 9'3  | 9'4  |
| 7'1  | 8'1  | 8'2  | 9'1  | 9'2  | 9'3  | 10'1 | 10'2 | 10'3 | 10'4 |
| 8'1  | 9'1  | 9'2  | 10'1 | 10'2 | 10'3 | 11'1 | 11'2 | 11'3 | 11'4 |
| 9'1  | 10'1 | 10'2 | 11'1 | 11'2 | 11'3 | 12'1 | 12'2 | 12'3 | 12'4 |
| 10'1 | 11'1 | 11'2 | 12'1 | 12'2 | 12'3 | 13'1 | 13'2 | 13'3 | 13'4 |
| 11'1 | 12'1 | 12'2 | 13'1 | 13'2 | 13'3 | 14'1 | 14'2 | 14'3 | 14'4 |

The determinant of the entire matrix, which is of the tenth order, is the criterion for a quartic curve. The determinant of the minor of the sixth order, comprised within the first six lines and columns is the criterion for a cubic. The determinant of the third order, comprised within the first three lines and columns (subject to a remark about to be made) will furnish the criterion for a conic, and the apex of the matrix is the criterion for the straight line. By adding on five more lines and columns, according to an obvious law, the matrix may be extended so as to give the criterion for a quintic; then six more lines and columns a sextic, and so on as far as may be required.

The remark to be made concerning the determinant of the third order referred to is that it contains the irrelevant factor  $2'1$ , i.e.  $\frac{y''}{2}$ , so that the criterion for a conic (Monge's)

is this determinant divested of such factor. It is *certain* that the next determinant is indecomposable, and is therefore the criterion for a cubic. There is no reason that I know of to suppose that any other determinant except that one which corresponds to the conic, is decomposable into factors. If this is made out, then, observing that the single term which is the criterion for the right line is indecomposable, we have another example of what may be called, in Babbage's words, a miraculous exception to a general law.

A well-known similar case of such miraculous exception I had occasion many years ago to notice in connection with the criteria for determining the number of real and imaginary roots in an algebraical equation. Such criteria may, with one single exception, be expressed